

Interactive comment on “Macroalgae contribute to nested mosaics of pH variability in a sub-Arctic fjord” by D. Krause-Jensen et al.

Anonymous Referee #1

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Krause-Jensen et al. measure pH, temperature and oxygen concentration across several scales where pH is expected to vary naturally due to macrophyte metabolic activity. The measurements in this manuscript are comprehensive and impressive, but while some are not novel (it is well established that pH varies due to macrophyte photosynthesis both on a habitat wide scale and at their thallus surface in the diffusion boundary layer), this manuscript will still be of extreme interest to members of the scientific community who study small-scale coastal biogeochemistry, benthic ecology, macroalgal physiology, ocean acidification, and any combination of these themes. What is particularly significant about the manuscript is the compilation pH variability caused by autotrophs at a variety of scales, and even more so, the investigation of pH variability several heights above the substrate within the kelp bed is particularly novel/interesting. These two aspects of the manuscript are extremely useful to the scientific community.

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As the authors state, measurements such as these are important for forecasting the effects of ocean acidification on future shallow coastal systems. Most critiques I have of this manuscript are of a relatively minor nature.

Moderate comments: 1) The use of saturation state throughout: If total alkalinity or dissolved inorganic carbon was not measured during specific seasons, then I consider it is inappropriate to calculate saturation states from pH and salinity for these sampling periods, regardless of whether correlations between salinity and total alkalinity are known from this region. Since pH and saturation states are so closely correlated, I do not consider that also mentioning and showing rough estimates of saturation data states adds anything to the manuscript. Furthermore, I consider it somewhat simplistic to imply that saturation states below 1 are "corrosive" (e.g. line 51). There is much evidence that this is not the case.

2) Microprofile methods: Many details are missing with respect to the measurements in the DBL: How long was the micro-electrodes left before the measurements in the DBL began? I.e. was the DBL in steady state or not? If the DBL was not in a steady state then the pH data obtained could underestimate the true values that can be reached (i.e. as time goes by pH at the surface should constantly increase until the steady state is reached). What were the seawater flow velocities used here? Velocity is one of the most important components that modify the pH within the DBL. What was the dimensions of the chamber used during these measurements of pH, and how was flow velocity modified? How many replicates were conducted with each species? If the aim was to determine what pH likely is at the surface of the different species in the field, then the authors need to demonstrate that environmentally realistic conditions were used. From the details here I cannot judge whether the data collected here reflects processes occurring in the real world - see comments below regarding discussion of these data also.

Minor comments: Introduction: 3) Line 78: The sentence that kelp modify pH "as demonstrated for subtropical and tropical vegetated habitats" is a little odd, as this

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manuscript deals with colder climates, but the introduction does not mention the fact that these types of measurements have been conducted before in colder ecosystems. Given that this manuscript is investigating the ability of macrophytes to modify pH in colder waters, and that the sentence itself is referring to the ability of kelp to modify pH (which predominately live in temperate and sub-polar ecosystems), I would add citations to two papers that deal specifically with the capacity of kelp to modify pH in a sub-Antarctic and temperate ecosystems (e.g. Cornwall et al. 2013a - referenced below, Delille et al. 2009), both papers which found large variability over a diel cycle. This is strange that the Delille paper is not cited here, as it is cited and discussed in the discussion.

4) line 106: The term "thallus boundary layer" should be changed to diffusion boundary and a citation that describes what this is and how it is formed is needed, as not all readers will be familiar with this.

Methods: 5) Study area: Kelp habitats are mentioned here and throughout the methods, but the specific species that are dominant in the study area should be given here; are they the same species investigated in the micro-scale pH measurements? The same comment applies for the macroalgal-dominated intertidal regions. The same comment applies to the figure legends containing photographs of seaweed, these need to have species names on them.

6) The study area describes the general study area well, but specific details of the deployment area of diurnal variation in the kelp bed are needed, in particular with respects to depth and species composition where the deployments took place, as both would likely influence pH. Also, the description of the deployments within and outside kelp beds are somewhat ambiguous as to whether there is spatial pseudo-replication occurring, i.e. are the 3 kelp bed deployments closer to each other than the 3 non-kelp bed deployments? If the deployment locations of pH sensors within and outside of the kelp forests are segregated spatially, then I question whether it is appropriate to test for differences between them. 3 different kelp beds in different locations should have

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been used, rather than 3 locations within the same bed (as it is written currently).

7) Micro-scale pH variability: Not all readers will know what each of the six species of macrophytes are. Mentioning what each are (i.e. Ochrophyta, Rhodophyta etc.) would be helpful.

8) Were there any effects of cutting the macroalgae on the pH data measured? It is known that leached substances from some, but not all, kelp species after they are wounded can reduce pH.

9) Lines 219 - 221: In nature the macroalgal blades do not exist in isolation, yet here they are examined in this way. Kelp canopies can attenuate water (as mentioned by the authors in the discussion), is it not likely that this could further increase the DBL thickness, leading to larger changes in pH at the thallus surface? Some discussion of how this set-up could influence the results should be mentioned.

10) Line 236: The term "DBL" is defined previously and should be used throughout rather than the more colloquial "boundary layer".

11) Line 216: Some mention of how the different species of macrophytes' blade varied in morphology might be useful here, as DBL thickness can be altered by even small undulations (Hurd and Pilditch 2011).

Results: 12) Figure 7: I consider this the most novel aspect of the study, but it is hard to see the exact differences the authors mention in the results. Is it possible to break this figure down in a second panel that displays the mean of each day, say every hour or so, so that the mean and variability of pH at each time of the day in each location can be observed?

13) Lines 329 - 331. This is more a discussion point, but begs the question of why the DBL thickness is not presented, or why photosynthetic rates were not measured? DBL thickness should have been easy to calculate with the methods used here to determine pH within the DBL.

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Discussion: 14) Lines 363-366: The differences in pH between kelp, and non-kelp, dominated habitats recorded here were small in the paired measurements. In addition, no data is provided showing that the density of kelp influences pH in a particular habitat, nor do the authors conduct manipulative experiments that separated out the effects of kelp and phytoplankton on pH variability. Therefore, I would not consider that the manuscript can support the statement that "mosaics of pH reflected that the density of primary producers...were key drivers of pH variability".

15) Page 16, 2nd paragraph: Comparing pH variability here with that in other systems is really like comparing apples and oranges unless a multitude of factors are examined. Different depths, seawater retention times, densities of macroalgae, light regimes, species, etc could all play important roles, making comparisons difficult. The start of this paragraph needs an overhaul, there are a number of unreferenced points, the studies the authors compare their data to are not fully inclusive, and overall I consider that the paragraph should make more of an effort to compare the data here to points I have mentioned here, rather than speculating on why there was a slight difference (0.03 units) between the filamentous and kelp habitats.

16) Page 17, 2nd paragraph: The first half of this paragraph begins to discuss points of extreme importance to those scientists who study macroalgal habitats. This should be expanded and a separate paragraph should deal with the variability in rockpools, which is a phenomenon that is well known and of less importance to the readers.

17) Line 418: Regarding pH measurements of *Sporolithon durum*, the review of Roleda and Hurd (2012) should not be cited here, they reproduce the exact figures from Hurd et al. (2011) which is the original source.

18) Line 419: The citation to Cornwall et al. (2013) is not in the bibliography, but rather the paper in the bibliography is Cornwall et al. (2012). I suspect that Cornwall et al. (2013b -referenced below) is required in the bibliography. Please check all other references are correct.

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19) Line 407-408 & Figure 8: I question why pH did not reach a high value for *Ulva* here, when it is known that *Ulva* has some of the most efficient CO₂ concentrating mechanisms known, and is capable of elevating pH to very high levels in enclosed habitats – as mentioned by the authors. The authors should discuss the possible reasons why pH elevation in the DBL was not high in subsequent sections.

20) Page 19, 1st paragraph: Though high pH could be an important refuge from potential impacts of ocean acidification in the future during the day, what about at night when pH is even more reduced?

References cited in this review:

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Cornwall CE, Hepburn CD, Pritchard DW, McGraw CM, Currie KI, Hunter KA, Hurd CL. 2012. Carbon-use strategies in macroalgae: differential responses to lowered pH and implications for ocean acidification. *J. Phycol.* 48:137-144.

Delille B, Borges AV, Delille D. 2009. Influence of giant kelp beds (*Macrocystis pyrifera*) on diel cycles of pCO₂ and DIC in the Sub-Antarctic coastal area. *Estuar. Coast. Shelf Sci.* 81:114-122.

Hurd CL, Cornwall CE, Currie KI, Hepburn CD, McGraw CM, Hunter KA, Boyd P. 2011. Metabolically-induced pH fluctuations by some coastal calcifiers exceed projected 22nd century ocean acidification: a mechanism for differential susceptibility? *Glob. Change Biol.* 17:3254-3262.

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Hurd CL, Pilditch CA. 2011. Flow-induced morphological variations affect diffusion boundary-layer thickness of *Macrocystis pyrifera* (Heterokontophyta, Laminariales). *J. Phycol.* 47:341-351.

Roleda MY, Hurd CL. 2012. Seaweed responses to ocean acidification. Pages 407-431 in Wiencke C, Bischof K, eds. *Seaweed Biology: Novel insights into Ecophysiology, Ecology and Utilization*. Berlin: Springer Berlin Heidelberg.

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